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OF THE

ROYAL ASTRONOMICAL SOCIETY.

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WILLIAM LASSELL, Esq., President, in the Chair.

G. F. Rodwell, Esq., 14 Denbigh Place, Belgrave Road,
Francis Barrow, Esq., 3 Phillimore Gardens,
Howard Grubb, Esq., Dublin,
C. Bird, Esq., Bideford; and
John Sidebotham, Esq., George Street, Manchester,

were balloted for and duly elected Fellows of the Society.

The Index to the first twenty-nine volumes of the *Monthly Notices* is now ready for delivery to the Fellows in the same manner as the *Memoirs*, *i.e.* by application to the Assistant-Secretary at the rooms of the Society, either personally or by an authorised agent. The price to persons not Fellows is 5*s*. It is to be obtained of the Society's publishers, Messrs. Williams and Norgate, Henrietta Street, Covent Garden.

Note from the Astronomer Royal.

Perhaps I may be permitted to make use of the *Monthly Notices* as a channel for conveying, to our scientific correspondents of foreign countries, a notice of the danger to the packets transmitted by post, arising from the flimsiness of the paper in which they are enveloped. Some time since, an empty Italian envelope reached me; after some inquiry, the pamphlet which it had inclosed was found. Subsequently, an empty American envelope came; its contents have not been found, and their subject is not known to me. Lately, an envelope, indorsed as from an Italian meteorological observatory, and tied up in our Post Office, was found to contain a Belgian newspaper; probably some ardent politician has been disappointed on finding in his envelope a set of meteorological observations.

October 20th, 1870.

Note on the Zodiacal Light. By Richard A. Proctor, B.A.

It cannot but be regarded as a remarkable circumstance that the nature of the zodiacal light should in the present state of Astronomy continue to be a *quæstio vexata.* I do not here refer to the physical constitution of this object, respecting which we may possibly be unable for many years to form a satisfactory theory, but to the determination of the actual position of the zodiacal light in space. Astronomers have been able to determine from geometrical considerations the paths of such objects as comets and meteors; it would therefore seem that the position of such an object as the zodiacal light ought ere this to have been determined.

Yet it must be admitted that there are peculiar difficulties in this problem. We can reason respecting the distance and motion of a comet, because we know that our observations are made on one and the same body whose motions are in accordance with the laws of gravity. It is otherwise with respect to the zodiacal light. We see a certain glow or radiance occupying a definite position with respect to the horizon and to the celestial circles; but we have no means of ascertaining whether the objects from which that radiance proceeds are the same at any one time as at any other, or indeed (as will presently appear) whether a single one of the constituents forming the zodiacal gleam at one season is present within the same region of the solar system at another.

The geometrical considerations applicable to the zodiacal light are, however, too definite to admit of question,—in other words, the path to be followed in seeking for a theory of this object is unmistakable. Hitherto, so far as I am aware, that path has not been traced out *far enough* for the attainment of definite views,—the perplexities which presently surround us as we follow it having seemed perhaps to render further research hopeless.

It happens, however, not unfrequently, that the very difficulties surrounding a subject of this sort assist us,—in this way—that they enable us to reject theories which otherwise might engage our attention and so cause perplexity. Precisely as the very complexity of a lock makes us all the more certain that a key which opens the lock is the key really appertaining to it; so where a subject of astronomical research presents many perplexing phenomena these become so many reasons the more for accepting a theory which is not contradicted by any one of them.

This is, I think, the case with the zodiacal light. We are able—as I hope now to show—by considering the peculiarities of this object, to get rid, one after another, of various theories which might otherwise distract our attention. And though by this process of elimination we may not be enabled to determine quite the true theory of this object, we can yet considerably narrow the field within which selection has to be made.

The first considerations to be dealt with are those which depend on the normal features of the zodiacal light. It is well known that the light exhibits usually the figure of an oblique conoid whose axis lies close by the ecliptic, and whose vertex lies at a varying distance from the position of the Sun. Near the axis the light grows brighter, except close by the vertex, where it is even fainter than at the other parts of the border. The following table, prepared by Herr Klein, from modern observations, indicates the varying range of the vertex from the place of the Sun,—though it must be remembered (and will be recognised at once by every one familiar with the varying position of the ecliptic during the year, and other like circumstances) that these measures indicate variation in the extent of visibility rather than (of necessity) any real variation in the extent of the light.

Day of the Year.	Distance of Vertex from Sun.	Part Observed.
January 2	83°·0	Western half.
26	91·8	,,
February 11	81·0	,,
March 14	74·0	,,
April 14	75·0	
May 4	65·0	,,
August 1	77·0	,,
September 15	58·0	Eastern do.
October 17	74·5	,,
November 12	71·3	,,
29	56·5	,,
December 13	61·0	,,
28	80·5	,,

The setting of the zodiacal light when the western half is visible, and the rising of the light when the eastern half is visible, take place quite regularly, and in a manner precisely corresponding with what would be observed if the zodiacal light were a distant object like a planet, a star, or a portion of the the Milky Way.

Now these circumstances at once enable us to reject the theory that the zodiacal light is a terrestrial appendage — by which I understand for the moment an object lying within the Earth's atmosphere. For there can be no question whatever that if any definite portion of our atmosphere were rendered luminous in any way, that portion would either occupy an unchanged position, or would shift according to the laws regulating the process of illumination, or according to the winds, or other like terrestrial causes. Now that on any given occasion such causes might so operate as to give the illuminated air the appearance of rising or setting as celestial objects do (that is, not *merely* rising or setting, but rising

or setting along declination parallels) is quite possible, however unlikely. To take an illustrative instance: a balloon, seen at any one instant between an observer and the Sun, might be carried by the winds so as to continue between him and the Sun, even until the hour of sunset. But to suppose that night after night at any station a relation so peculiar would characterise the illuminated air, is like supposing that a balloon, started day after day from a given place, would day after day fulfil the condition considered above. This is obviously incredible. But even if it were credible, it would be insufficient, since the region of our atmosphere which would have to be illuminated in order to account for the zodiacal light as seen in one place, would, as seen from other stations, present an appearance wholly different from that of the zodiacal light. In fact, if the former place were in England, the zodiacal light would actually be in the zenith of places 900 miles or so west or east of England.

Next we have the normal aspect of the zodiacal light in different latitudes to consider. Now we have the most positive assurances from astronomers of eminence that the zodiacal light, wherever seen, occupies ordinarily precisely those regions of the heavens corresponding to the theory that it is too far from the Earth to have an appreciable parallactic displacement. We have the evidence of practised astronomers like the Astronomer Royal for Scotland, Captain Jacob, and others; and all the evidence we have points to the conclusion that the zodiacal light, as seen in the tropics, extends at any moment over those same parts of the stellar heavens which it illuminates as seen from our northern stand-point. The *limits* of the light may seem greater in those latitudes than in ours, but the axis of the conoidal gleam is situated precisely as with us.

Now it seems wholly unquestionable that this quality of the light should dispose at once and for ever of the theory that the zodiacal light is due to the existence of a ring of matter around the Earth.

Let it be remembered that there is only one way in which the ordinary aspect of the zodiacal light can at all be interpreted on such an hypothesis. If there were a ring of meteorites as far from us as the Moon is, then undoubtedly there would be a gleam in the west after sunset, and in the east before sunrise, in the position where we see the zodiacal light. And further, the individual meteorites producing any portion of that gleam would undoubtedly rise and set much as the zodiacal light is observed to do. But there would also be a gleam, and a much brighter gleam, in the south. The meteorites rising and setting would turn only a small portion of their illuminated faces towards us, those in the south (on or close by the ecliptic) would be "full," so to speak, and their combined lustre would be proportionately more considerable. Now supposing the ring exactly coincident with the ecliptic, the Earth's shadow would fall on the part due south. But the width of this shadow would (on the suppo-

sition we are considering) be relatively small. At midnight, in our latitudes, we should undoubtedly, on this supposition, see two arms of light extending from the eastern and western horizon along the ecliptic, each growing brighter and brighter towards the south; and a relatively narrow black rift would lie between the bright extremities of these arms. It is no theory that this would be the case, but a simple deduction from the most obvious geometrical laws.

If, then, we are to have a ring round the Earth, it must lie far within the Moon's orbit, so that the Earth's shadow may be wide enough to cover the meteorites along the whole of that long arc which under ordinary circumstances is undoubtedly unilluminated. The Earth's shadow cannot be more than 8000 miles across *anywhere*, and we must have our ring at such a distance that this width of 8000 miles may correspond to (or subtend) that wide arc of darkness actually observed under ordinary circumstances. (It is absolutely essential that ordinary circumstances should be accounted for; only when this has been done need we begin to inquire into extraordinary circumstances.)

Now we need not leave our own latitudes to decide how far off the ring should be to account for the apparent dimensions of the zodiacal light; because on the theory that the Earth's shadow, falling on a ring of some sort, defines the limits of visibility of the light, it would follow, precisely as in the case just considered, that the light would grow brighter and brighter up to the very edge of the shadow. (Supposing that edge to correspond to the extent of the Earth's shadow, there would be a somewhat ruddy bordering; but up to the commencement of that fringe there would be a regular increase of brilliancy.) But passing over this consideration (and also the consideration that the observed aspect of the zodiacal light in our latitudes is wholly inconsistent with the aspect thus shown to be due to the hypothesis we are dealing with), we may take as most favourable to the hypothesis of a meteoric ring near the Earth those observations of the zodiacal light in tropical regions which give to the ordinary apparitions of the light the greatest observed extension from the Sun.

We have it on the authority of Professor Piazzi Smyth that, even when he observed the zodiacal light under exceptionally favourable conditions, from an elevation, namely, of no less than 11,000 feet above the sea-level, the western tongue had completely set fully four hours before the eastern tongue began to rise. Now even if the eastern tongue were just beginning to rise when the western tongue had fully set, there would still be an arc of 180° between the two vertices.* But the shadow of the Earth would not account for such an arc as this between the vertices, unless the outer part of the ring had a radius not exceeding $\sqrt{2} \times$ radius of the Earth (even in the most favourable

* It must be remembered that each vertex, as the zodiacal light was seen by Professor Smyth, lay close by the ecliptic.

case of a station near to the equator), and with such a radius as this the outer part (even) of the ring would be always invisible from places having a higher northerly or southerly latitude than 45°.

And even if we set this demonstration on one side for a moment, it is yet obvious that whether a ring lying relatively near the Earth coincided in plane with the equator, or with the ecliptic, or with any intermediate plane, it could not possibly exhibit any approach to coincidence with the celestial ecliptic, when viewed from high latitudes. Further, as seen from high northern latitudes, such a ring would always have a parallactic displacement causing it to lie to the south of its geocentric position, and *vice versâ:* whereas no such association between the latitude of the observer and the apparent position of the zodiacal light has ever been observed ; far less such a systematic association as the case requires.

It is geometrically impossible, then, that the ordinary aspect of the zodiacal light can be accounted for by any theory which represents it as due to a ring of light-reflecting bodies around the Earth, whether that ring be close by the Earth or at a distance comparable with the Moon's.*

We need not consider the theory that the light may be due to a self-luminous ring around the Earth, for obvious reasons.

Now, passing from the normal features of the ring to more or less exceptional peculiarities, we find ourselves compelled to reject yet one other theory of the light—I mean the theory that it is due to a disk of minute bodies travelling in orbits of small eccentricity around the Sun.

The peculiarities which oppose themselves most strikingly to this theory are those which relate to the position and extent of the zodiacal light, though it will be obvious that the observed variations in the apparent brightness of the light are not readily explicable on this hypothesis.

Admitting the existence of a disk of bodies, travelling as supposed, it will be evident that the changes affecting the motions of any member of the system would correspond exactly to those

* While dealing with the relations presented by the Saturnian ring-system, in 1864, I was led to apply the formula, with suitable changes of elements, to the case of a ring circling the Earth; being invited to the inquiry by the perusal of the observations made by Lieut. Jones, and comments made thereon by Baron Humboldt. I found that there is not a single hypothesis as to the dimensions of such a ring which would lead to results according with or even in the slightest degree approaching the results of observations made upon the zodiacal light. This conclusion is embodied in a note at p. 117 of that treatise ; in which note I remark that such investigations "prove that the zodiacal light cannot be due to a ring of a minute satellite surrounding the Earth, the appearance of the ring in high latitudes being altogether different from that which would be presented by a ring surrounding the Earth." I am careful to refer to these researches and their results, because remarks have been published implying that I have somewhat hastily come to a decision on the points here dealt with. A complete mathematical investigation of the subject, made fully six years since, may be regarded as fairly meeting those remarks.

which would affect the motions of any considerable orb travelling at a similar distance from the Sun. In other words, the changes would resemble those slow periodic changes which affect the orbits of the Earth, *Venus*, and *Mercury*. Nor is it conceivable that the members of the system would so interfere with each other's motions as to affect appreciably at any time the appearance of the disk. Now changes such as these, affecting the individuals of a set of bodies which at any one time were spread with a certain uniformity (as the ordinary appearance of the zodiacal light would imply to be the case with its constituents) could not account for the observed changes in the position and extent of the light. The axis of the gleam has been seen at times, by practised observers, inclined at a considerable angle to the plane of the ecliptic. The extent of the zodiacal light has varied at times in the most remarkable manner, while its luminosity has been so variable that sometimes for months together it has been scarcely perceptible (in our northern latitudes); while at others it has been singularly conspicuous. I set on one side for the moment those observations by Lieut. Jones which would imply that at times the zodiacal light increases so greatly in extent as to become visible at once both on the eastern and western horizon. I also set on one side those observations by M. Liais according to which the zodiacal light can be seen at times extending as a complete arch from the eastern to the western horizon. Assuming these observations to be reliable (and those by M. Liais do not seem open to question), a true theory of the zodiacal light may be expected to account for them. But without insisting on this, it is evident, I think, that the admitted variations of the zodiacal light, in position, extent, and splendour, do not admit of being interpreted by the theory that the light is due to a disk, including always the same materials moving in orbits of small eccentricity.

Nor do our difficulties seem removed if we assume that the constituents of the disk travel in orbits of considerable eccentricity, so long as we suppose that the actual constitution of the disk is constant, or nearly so, amidst whatever variations in the distribution of individual constituents.

Yet the general aspect of the zodiacal light, and the considerations already applied to other theories, suffice to prove that there is always present around the Sun as centre a disk, whether composed of discrete meteorites, of vaporous masses, or of some combination of these and other forms of matter. The materials of this disk must be in motion around the Sun in accordance with the laws of gravity; at least we have no evidence whatever inviting us to the supposition that they differ in this respect from all the other constituents of the solar system.

We are thus led to the conclusion that the bodies composing the zodiacal light travel on orbits of considerable eccentricity, carrying them far beyond the limits of what we may now term the zodiacal disc. The constitution of the disc thus becomes

variable, and that within limits which may be exceedingly wide. They must be so in fact, if all the recorded variations of the zodiacal light are to be accounted for. In other words, it is requisite (if our evidence is to be explained) that the paths of the materials comprising the zodiacal light shall be not only for the most part very eccentric, but that along those paths the materials should not be strewn in such a way that a given portion of any path is at all times occupied by a constant or nearly constant quantity of matter.

According to this view the constituents of the zodiacal light would—at least as respects distribution along their several paths and the general figure of those paths—resemble very closely the meteoric systems which, as we know, the Earth traverses in the course of her annual motion around the Sun.

By considering the zodiacal light we have thus been led to a theory involving, and associated with, the theory of meteor-systems as now established by the labours of Adams, Leverrier, Schiaparelli, and others. But it is worth noticing that by reversing the process, and considering first the theory of meteor-systems so established, we are led quite as readily to the theory that there must at all times exist in the Sun's neighbourhood a disk of discrete constituents which would present precisely such an appearance as the zodiacal light. I have shown elsewhere that this result is a simple mathematical deduction from the evidence.

But setting this consideration wholly on one side, the fact remains that all other theories of the zodiacal light—that is, of the motions of its constituent parts, without reference to its physical constitution—have been eliminated. It remains only to be shown that this theory is controverted by no peculiarities in the observed appearance of the zodiacal light, and also that we should inquire what further general laws, if any, may be predicated of the motions of the bodies composing this object.

The fact that the axis of the zodiacal light is ordinarily close to the ecliptic, is accounted for on the assumption that the various paths along which the constituents of the zodiacal disc travel, tend to aggregate towards the neighbourhood of the ecliptic. There is nothing, however, to prevent individual systems from having a considerable inclination to that plane.

The observed variation of the zodiacal light in brilliancy, position, and extent, is obviously to be expected according to the view of its structure now under consideration.

The simultaneous appearance of an eastern and western light and Liais's observation of a complete arch of light, have to be accounted for as highly exceptional, but at the same time recognised phenomena. It is easy to see that both these phenomena may be regarded as indicating the occasional but very exceptional extension of the zodiacal disk to a considerable distance beyond the orbit of the Earth. But it must not be concealed that there are grave difficulties to be removed before this interpretation can be regarded as satisfactory.

Let us suppose, for instance, the case of a thin luminous disk occupying the whole orbit of *Mars*, and that the Earth is in the part of her orbit where her distance north or south of this plane is greatest. Then it will be evident that the outline of the disk as seen from any part of the Earth would correspond very nearly to a great circle of the heavens, and that the whole of the visible heavens south or north of that great circle would be hidden by the luminous disk. In other words, a region of the heavens far larger than that occupied by the arch of Liais, or by the eastern and western lights of Jones, should be occupied by the zodiacal light if it had some such extension as we have assumed in the case of this luminous disk.

It is to be remembered, however, that assuming (as we are bound to do) a considerable degree of flatness in the actual figure of the zodiacal disk, and more especially of its more distant portions, then much more light would be received from those parts towards which the line of sight is directed at a considerably acute angle, than from those parts which the line of sight crosses nearly at right angles. And it is easy to see that on any reasonable assumption as to the range of zodiacal substance which it is necessary that the line of sight should traverse in order that any appreciable light should be received, the occasional visibility of the light where the superior planets alone can be seen becomes as readily explicable as the ordinary visibility of the light in those parts of the sky where the inferior planets become visible.

It will be seen that all that can be strictly said to have been demonstrated in this paper is the fact that the zodiacal light is associated with the Sun, and not with the Earth ; that it is not due to solar light reflected from bodies travelling within the Earth's orbit, whether in circular or elliptic orbits, and that if the major part of the zodiacal light is reflected solar light, then the paths of the bodies reflecting that light must resemble those of the meteors encountered by the Earth. As the spectroscope seems to show that at least a portion of the light* of the zodiacal gleam is not reflected solar light, we cannot, in the present state of our knowledge, definitely decide on a theory as to the motions of the bodies to which the light is due. For the solution of the problem is obviously bound up with the interpretation of the physical nature of the zodiacal light. If some solar action, for example, rouses luminosity in certain definite directions—as, for instance, near the plane of the Sun's equator—in some such way as light is caused to appear along radial lines through and

* I use this mode of speaking not by any means as doubting the accuracy of Angström's observation; but because even if the greater part of the light gave a continuous spectrum, yet this spectrum might remain undiscernible even when bright lines corresponding to a very minute proportion of the total light were seen with ease. Nay, such bright lines as Angström found in the spectrum of the phosphorescent light from the sky might be detected when a continuous spectrum from the much brighter light of the zodiacal radiance remained unseen.

beyond the heads of comets, our power of theorising from such considerations as have been dealt with in this paper would be limited. It would still remain certain that the zodiacal light is not a terrestrial appendage (either near or far off), but what sort of solar appendage it might be would be a problem as difficult to solve as that presented by comets.

If the radiated structure of the Sun's corona as seen under favourable atmospheric conditions should be confirmed as more than an optical phenomenon, it is not impossible that we might be put in the way of interpreting the zodiacal light.

On the Performance of a Zenith Sector constructed for the Great Trigonometrical Survey of India by Messrs. Troughton and Simms, from the Designs of Lieut.-Col. A. Strange, as reported by Capt. J. Herschel, Royal Engineers. By Lieut.-Col. A. Strange, F.R.S.

I have from time to time informed the Society of the progress in construction of an extensive series of large instruments designed by me for the use of the Great Trigonometrical Survey of India, — comprising a Great Theodolite with 3-foot horizontal circle, two 5-foot Transit instruments with Clocks and Chronographs for determination of longitude, two Zenith Sectors with 4-foot telescopes for determination of latitude, and various other minor apparatus.

Of these, one of the Zenith Sectors (No. 2) has reached its destination and has undergone the test of a season's work in Southern India in the hands of Capt. J. Herschel, Royal Engineers, some extracts from whose report on its performance may be of interest to the Society. First, however, I will submit a brief description of the instrument.

Students of astronomical history are familiar with the zenith sectors employed by the elder geodesists. Ramsden's Sector, minutely described in Pearson's *Astronomy*, may be taken as a type of these. It consisted of a telescope nearly 8 feet long, suspended by the object-glass end from the top of a wooden framing 12 feet high, the eye end, armed with a micrometer tangent screw, traversing a short fixed graduated arc or sector. This instrument displays great ingenuity, particularly in the illuminating appliances. Even at the present day it is well deserving of study. It was, however, exceedingly cumbersome in transport, and, as I have often heard the late Sir George Everest say, a most troublesome instrument to adjust and use. That distinguished surveyor substituted for it powerful Alt-azimuths, two of which were employed in India from his time until recently for latitude observations. In about 1836 a Zenith Sector was designed by the present Astronomer Royal for the Ordnance Survey, in which work it has been extensively employed. The "Airy Zenith Sector" is fully

described, with illustrations, in the volumes of the *Ordnance Survey*. Its form is in keeping with modern ideas of astronomical instruments, and it constituted an immense advance on all previous sectors.

Some years ago Sir Andrew Waugh, the late Surveyor-General of India, and afterwards Colonel J. Walker, his successor, the present head of the India Survey, determined to have the latitude observed at far closer intervals in the triangulation than had heretofore been thought necessary; their object being to elucidate the obscurity surrounding the phenomenon of local attraction, an obscurity which still tends to affect with doubt all geodesical elements. Both these officers were of opinion that the sector principle offered advantages for taking these observations, and two Zenith Sectors were accordingly sanctioned by the Government of India, who called upon me to design them.

The form of the Airy Sector, or some modification of it, would naturally have been adopted, had it not been expressly pointed out by Colonel Walker that the weight of that instrument (1140½ lbs. without the packing-cases) rendered its transport in mountainous districts devoid of roads an absolute impossibility. It was, however, necessary that I should avail myself of such ideas as might be suggested by an instrument designed by so eminent a master, and employed with such excellent effect in the the English Survey. Sir Henry James was good enough to place it entirely at my disposal, permitting me to take it to pieces and to observe with it at Southampton under the auspices of Serjeant-Major Steel, who had worked with it extensively. The conclusions to which this study of the subject led me were — First, that I must entirely abandon the principle of Mr. Airy's design; but that, secondly, his method of observing was so perfect, that any future sector must be suitable to the employment of that method.

The Airy Sector revolves azimuthally between two fixed external points. These points are supported by two lofty cast-iron pillars, the weight of which is a large portion of the entire weight of the instrument. It was evident that a self-contained vertical axis would require far less ponderous masses. This was, therefore, the basis of my plan.

The arrangement ultimately adopted by me was, in general terms, as follows:—A wide based cast-iron column about two feet high contains within it a strong conical steel vertical axis whose lower extremity, of hemispherical form, works in a cup adjustable laterally for the purpose of levelling the instrument. The upper end of the vertical axis (omitting details) carries a cradle containing two bearings for the support of the horizontal axis. The cradle also supports four micrometer microscopes for reading off the sector divisions. The horizontal axis carries at one end the sectors 3 feet in diameter, and the telescope of 4 feet focus, at the other a counterpoise. When the telescope is directed to the zenith, a diameter passing through the centres of the two sectors is horizontal, whereas in Airy's Sector this line is vertical.

The four sector micrometers are arranged conically and the divided face of the sectors is bevelled, by which means a single light, fixed about 5 feet from the centre of the instrument, illuminates all four micrometers, very much in the same way as that employed in the Greenwich transit-circle. The instrument has no horizontal circle whatever, but has two stops which admit of its being reversed promptly in azimuth as is the case with the Airy Sector, so that stars can be observed twice at the same culmination, in order to eliminate collimation and other instrumental errors.

Some conception of the instrument may be given by saying that it bears a general resemblance to an Equatoreal of the so-called German form set up as it would be for use at the pole, the vertical axis of the sector corresponding to the polar axis of the Equatoreal, and the transit axis to the declination axis.

It would be impossible to render the details of the instrument and its numerous adjustments intelligible without many illustrations, for entering on which extensive work my present duties leave me no time. I hope at some future period to execute this task in the fullest manner.

It should be mentioned that the method of determining latitude known by the several names of the "differential," the "zenith telescope," and the "American"* method, having of late years attracted much notice, I felt it right to supply my Zenith Sector with the appliances required for this method, in order that the two rival systems,—the differential and the absolute,—might be tested with the same identical telescope and mounting.

Before ordering the work to be commenced I submitted my designs for the instrument to the Astronomer Royal, who was good enough to examine them, down to the minutest details, with great care and attention. The general approval which he accorded to them gave me a confidence in their soundness which I could not otherwise have felt. He, however, considered that the gain in lightness claimed for my instrument, which I had assumed from mere inspection of drawings of his sector and mine to the same scale, should be strictly ascertained. I, therefore, computed the weight of each separate part from my drawings, a task which occupied me incessantly for a fortnight. I found the total weight by computation to be 600 lbs., against 1140½ lbs., the weight of the Airy Sector. So far the problem of designing a much lighter instrument with equal optical and measuring power appeared to be solved, the questions of convenience in use and efficiency of performance remaining to be tested. It is a curious, and in some degree I believe an accidental fact, that the instrument when completed and actually weighed was found to be 595½ lbs., that is, within 4½ lbs. of the computed weight.

The instrument, after undergoing very strict examination and

* This I believe is an incorrect appellation. The method is indicated in Pearson's *Astronomy*, and is said to have been first used by English observers.

actual use in determining latitude at Lambeth Observatory, and after receiving many alterations and additions, was at length despatched to Madras in October 1869 by the overland route, under the special charge of Lieut. Rogers, R.E., an officer of the Survey, who had thoroughly mastered its principles under my instructions. It arrived at Bangalore in November 1869 in perfect safety, and was immediately employed in field operations by Capt. J. Herschel of the Survey Department.

I was naturally most anxious to hear how the instrument had performed, for I well knew how many parts of it admitted of doubt from their novelty, and how many others I now saw ways, as I believed, of improving. Capt. Herschel, with a consideration I shall never forget, did not suffer me to remain long in suspense. Immediately on the conclusion of his season's work, whilst still engaged in field duties, he wrote me a letter, commenced "Coimbatore Base, 20th March, 1870," and concluded "Bangalore, 4th April," from which I will now make a few extracts. He says,—

"I am now observing with your Sector at my sixth station, since receiving it in the end of November last. In the interval I have taken about a thousand zenith distances, or perhaps twelve hundred. I cannot spare the time to count them up now. I am, therefore, in a position to speak without hesitation as to the facility and comfort with which the work can be performed. . . .

"Of course you will prefer a perfectly honest judgment even if the approval is not altogether unqualified. But you need not be afraid; for, in truth, I shall find it difficult to find fault. The ease with which the observations can be taken is proved by the fact that I now make five minutes my limit in choosing the Star-list — not that that is the *smallest* interval allowable, but the interval at which I can, if called upon, take three or four stars in succession. It could be done quicker, but at some sacrifice of precision. You know exactly the work that has to be done in that time, so I need say nothing to bring out the force of the evidence. There can be no better proof," Capt. Herschel adds, "of forethought and efficiency of design and execution in an instrument, than that it allows of rapidity, and certainty, and comfort in manipulation, and spares the observer mental and physical distractions and distress."

The facility of manipulation here spoken of by Capt. Herschel will be rendered intelligible by the statement that in the brief interval of five minutes no less than twenty-four distinct operations, consisting of settings, readings, intersections, and reversals, have to be performed. The testimony to this feature in the instrument is peculiarly gratifying to me, as I have always held that convenience in use is an absolute essential in an instrument intended for taking long series of observations, and that the designer should spare no pains to render the tedious labour of the observer as easy as possible. According to my experience this maxim does not always receive due attention. But gratifying as

the commendation is to myself, I should have shrunk from citing it if it did not reflect at least equal credit on the makers of the instrument, Messrs. Troughton and Simms, and on the skill of Capt. Herschel in using it.

As to permanence of adjustment, Capt. Herschel writes:—

"I am using the microscopes *as I found them;* that is to say, I have re-adjusted nothing as to run and focal distance; simply because there was no occasion to do so. This proves," he adds, "that these adjustments are of a nature to stand a good deal of travelling."

With regard to the sectors, Capt. Herschel says:—

"The graduation seems to be almost perfect. I question," he remarks, "whether the error of position of any division exceeds $\pm$ 2″, as compared with the mean of all."

On the subject of instrumental and observational error the following passage occurs:—

"My observation record is made to show the *zero error of microscopes* at every observation—generally before passing on—so that mistakes may be detected on the spot. This is a joint test of graduation, intersection, and observation errors, as well as of stability; and it is satisfactory to find that the extreme range is not greater than $\pm$ 1″ for six or seven hours' work."

The consistency of the instrument with itself, or its power of repeating, is thus reported on:—

"I take fifty stars at each station, three times each. I find that the zenith distances, corrected for dislevelment, agree with each other, so that all the members of each triplet, *in about three cases out of four*, fall within a range of 1″."

The accuracy attained in the final results is defined as follows:—

"At the last station twenty-two stars out of the list were *Nautical Almanac* stars, so that the latitude was easily deducible. Cutting out *Procyon*, which has a large proper motion, and, I suspect, an erroneous place; and γ' *Virginis*, which has almost certainly an erroneous place, due to confusion of the two components, the remaining twenty give latitudes all falling within a range of 1″·6."

"I may be too sanguine," Capt. Herschel goes on to say, "but I think I see here a glimpse of something *beyond local latitudes.* It is impossible, in the field, to go far in computations without risk, and I would not be over-confident, but the *order of accuracy* indicated by these flying reductions has no parallel in my experience,—a not very extensive one I know. I take no credit," Capt. Herschel adds, "on the score of observation. I attribute it wholly to the instrument, and believe it can do still better. As regards latitude only," he states, "I am satisfied that one night's observation (of thirty-six stars in six hours) will suffice to give a result whose probable error will be *not greater than one-fifth of a second.* [In the case I have mentioned," he adds, "it is $\pm$ 0″·11, but this is from twenty stars taken three times

each."] "If I am right in this, and the event will prove it in two or three months, your Zenith Sector will have to be promoted to the task of determining *places*, as well as latitudes."

Capt. Herschel points out various omissions and imperfections regarding which I entirely agree with him. He says the instrument will not give first-rate results as a Transit instrument, in which he is quite right. It is unsymmetrical, and has not, to use the apt expression of Sir John Herschel, to whom I sent his son's letter, sufficient "hold on the meridian." It will, however, determine time, when clamped in a manner I have since pointed out to Capt. Herschel, with an accuracy ample for observatory purposes, but certainly not for determining Right Ascensions.

I had particularly requested Capt. Herschel to test the differential method of latitudes, and on that point he writes:—

"I have said nothing about differential zenith distances; but the method has not been ignored. I have sufficient observations of that kind to give a pretty good idea (when they are worked out) of the relative advantages. My impression is that it is a waste of power, with such an instrument."

This opinion, guarded though it is, merits some weight as coming from Capt. Herschel, who, in a former letter written before he had used the instrument, made the following remark on this very subject: "I anticipate that the advantages of the N. and S. star transits (in pairs) will, in the long run, put zenith sector readings out of court."

I may, perhaps, be permitted to say a few words on this subject myself. The differential method has undoubtedly the great advantage of requiring a much less weighty, less complicated, and less costly instrument than the absolute method; it also practically eliminates refraction doubts; and the reductions are far more simple. But, on the other hand, the absolute method admits of a greater number of results being obtained in a given time,—a matter of no small importance in extensive geodesical undertakings in wild or unhealthy countries; and it also admits of a given amplitude being determined by the simultaneous observation, at the two extremities of an arc, of the same identical stars with two similar instruments worked by two sets of observers, on the very same nights. This plan was most successfully carried out by the late Sir George Everest on the Great Meridional Arc of India, and it has the invaluable merit of being entirely independent of the places of the stars observed. As to the relative accuracy attainable by the two rival methods, that time and very careful extensive series of observations alone can decide.

So far as my experience or knowledge extends, this is the first instrument of importance sent to India which was found fit and ready for immediate use on its arrival. This is to be ascribed to no particular skill on any one's part, but to the principle on which the system of inspection in my office is based,—namely, that no instrument shall be considered as complete until the very observations for which it is intended shall have been

taken with it. Seven years' experience has proved to me that no examination, however detailed and laborious, is equivalent to the above precaution.

I think it desirable to draw particular attention to this matter with reference to the extensive series of instruments now being constructed for observing the transit of *Venus*. Unless these are actually set up and used precisely as they will be for the observation of that phenomenon, some disappointment is almost certain to arise, whatever the skill and forethought of the most eminent makers may have done to secure success.

Aurora Borealis of 24 Sept., 1870. By Rear-Adm. Ommanney.

A brilliant display of the Aurora Borealis was visible here last night; I submit the following statement of my observations, which probably may be acceptable to the Society.

About 8.30 P.M. a broad white streak of light was seen due north extending from the horizon towards the North Star; this streak was soon accompanied by several others of considerable brilliancy, rising and streaming upwards from the horizon between the N.W. and N.E. points, and converging towards the zenith, at the same time all the northern portion of the sky became illuminated.

At 9 P.M. the coruscations of the Aurora became more brilliant and active, streaming with great rapidity towards the zenith, but none could be seen to reach that point, assuming very beautiful colours, which were generally of light pink and reddish hues.

About 10 P.M. the colouring and brilliancy of the Aurora attained their fullest effect, the coruscations varying with yellow, pink, and almost crimson colours, the intermediate spaces of the sky in the background appearing to be light green, the brightest streamers were confined to the north or north-westerly directions; the coruscations continued in rapid action with surprising grandeur up to 10.30 P.M. when a somewhat indistinct purple arch was visible, with its culminating point north, and about 35° above the horizon; shortly after this exhibition, the display of coruscations and luminous streamers gradually ceased action, but the northern portion of the sky remained illuminated, resembling twilight after sunset.

The sky was quite clear at the commencement of the Aurora, it became partially obscured about 10 P.M., the clouds, especially those to the N.E., reflected very bright roseate hues from the Aurora, which in combination with the varying colour of the coruscations produced a most striking and beautiful spectacle. Three shooting stars were observed between 9 and 10 P.M. and several were seen on the previous night.

The barometer read 30.40, and has not been below 30.30 during the past week; the temperature has been very regular for

the week, the maximum by day 64°, and the minimum at night 51°; winds have been constant from the eastward, the sky free from cloud.

The northern region of the sky remained illuminated throughout the night, producing a strong reflection on the sea, and rendering the colouring of the rocks and vegetation around me quite perceptible, even at 3.30 A.M. the light from the northward produced the effect of early dawn.

I must add that during all my Arctic voyaging I never witnessed in any Aurora the same conditions of varied colouring as were displayed on this occasion.

It may be of interest also to state, that on the 21st inst. I observed a bright meteor to shoot in a path from near the Polar Star to *Capella*, vanishing near the latter after discharging a brilliant light green flash.

From Ilfracombe the view to the north commands an extensive range of sea horizon.

Ilfracombe, Devon, 25th Sept. 1870.

On the Spectrum of the Aurora Borealis.
By John Browning, Esq.

During the display of the Aurora Borealis which occurred on the evenings of the 24th and 25th of October, I confined my attention to observing the spectra of the light, taking it in different parts of the sky. When the spectroscope was directed to the more luminous portions, which were generally of a silvery white, the spectrum appeared to me to consist of only one line. I could not succeed in verifying the position of this line, but it appeared to be situated between D and E in the spectrum. When observing the light of the red portions of the sky, a faint red line became visible. I had no means of verifying the position of these lines with any degree of exactitude, but I was able to throw into the field of view a faint continuous spectrum from a distant light, and also the bright yellow sodium line produced by a spirit-lamp.

The colour of the green line was very peculiar; had I not been able to observe it by comparison I could not have formed any idea of its position. It was an exceedingly light silvery green, or greenish grey, and often seemed to flicker. Besides the two lines I have particularly described, I occasionally suspected others, one in the red, and one in the blue, but I could not be at all sure of this. The colour of the light of the Aurora seen over the greater portion of the heavens resembled exactly that of the discharge of electricity from an induction coil through a vacuum formed from atmospheric air.

Further Observations on the Currents of the Solar Photosphere: Lateral, Ascensional, and Cyclonic. By C. H. Weston, B.A. Cant., F.R.A.S., F.G.S.

The remarkable extent of disturbance which the solar photosphere has lately exhibited must have arrested the attention of most telescopic observers. Towards the end of August there was a train of groups and spots (more or less remote) extending nearly across the upper part of the Sun's disk, over a surface of about 400,000 miles. Since this period the trains of spots, although contracted both in extent and numbers, have yet been very many, and have afforded fine objects for critical examination. But as it is only by minute and exclusive attention to one particular spot, and endeavouring when practicable (*de die in diem*) to connect in *sequence* its successive changes, both in form and direction, that we can hope to acquire any increased insight into solar physics. I selected, after a general survey, one large elliptical spot for continued study. It was in northern latitude and surrounded by groups of minor openings and ramified faculæ, betokening alike the actual presence of energetic activity and the promise of future development, and destined most fortunately to exhibit subsequently the interesting and somewhat rare phenomenon of vorticose currents. The examination of this spot began on the 20th instant; but before giving the details of this locality, I would make the following extracts from my note-book respecting three other days' observations, which, although disconnected in themselves, bear indirectly upon our present subject, in showing the apparent connexion of *ascensional* forces with the formation of solar maculæ.

1870, August 31. Near the Sun's western periphery a fine spot; the photosphere which formed the external boundary of the penumbra was marked by a circumscribing ring of elevated ranges, and the penumbra itself appeared to consist first of an uniform surface, and then, at the *margin* of the umbra, to rise again in upcast masses. The image of the solar disk thrown upon a white screen, and thus enlarged to two feet diameter, exhibited very clearly the same striking areographical features.* Here were evidently *upward* movements acting not only on the neighbouring photosphere, but also within the penumbral portions in immediate contact with the umbra.

Sept. 2. Another large spot; pear-shaped and possessing a very broad and flat penumbra; shut off light by diaphragms;

* Aperture of the Newtonian telescope used was nine inches diameter, which thus ensured a good amount of light for such enlarged views; and the same aperture was also used for direct vision, by excluding the intensity of the calorific rays (thus focalized), not only by coloured glasses but by diaphragms with differently drilled openings, taking care not to introduce serious optical errors by *too minute* perforations. The advantages of large apertures in solar observations are duly noticed in the valuable contribution to solar physics by Messrs. De La Rue, Stewart, and Löewy.

details well seen. Great inequalities were visible in the penumbral surface, quite similar in character to a disturbed region of the general photosphere, and differing only in light-reflecting powers.* Here also, therefore, appeared evidences of *upward* movement in the penumbra itself.

Sept. 8. Definition very good, Spots now few, but the photosphere strikingly corrugated. The undulations of light and shade dichotomizing and anastomosing in every direction, like a sunlit sea agitated by a strong stiff breeze.† Again struck with the general elevations *around* the solar spots.

Thus the photosphere around the openings, the actual penumbral surface, and even the penumbral coast-line bounding the umbra, equally exhibit apparent evidences of the evolution of forces from beneath, while in a former communication to the Astronomical Society I noticed evidences of *upward* movement over the umbra itself.‡ These phenomena seem unitedly to militate against the acceptance of the theory of a "downward current" of luminous matter (over umbræ), adopted by such high authorities as to make less experienced and qualified observers hesitate in suggesting a diverse opinion on this difficult point. Yet the designation of "craters" given to these maculæ by some solar physicists (if given in reference to their shelving and more or less approximately inverted conical form) would rather lead to a confirmation of the inference that such crateriform orifices resulted, as in volcanic agencies, from the gradually widening (or radiating) action of a subjacent focus of explosive energy.

I will now come to the consideration of the spot fixed upon for special and continued observation.

Sept. 20. A magnificent elliptical opening on the east limb, exhibiting, as before stated, evident symptoms of active elevatory movements. Its major axis (including penumbra) was not far short of 40,000 miles. Of this locality I had unusually distinct views during four successive days. I marked three straight promontories or tongues (A B C), which appeared attached to the inner north-eastern and northern margin of the penumbra, and projected at different lengths over the central umbra. Their relative positions were towards the N.E., N. and N.W. respectively. The most easterly (A) was very acuminated with a gradually widening base. The next in position (B) had a sharp, but rather short terminal, and continued towards its base with parallel sides. The other (C) towards the west was triangular in form. The apices of C and its neighbour B were near, but not in contact, while A was at some distance from B.

Sept. 21. To-day observed that C (about half way down)

* The light-reflecting powers of the penumbræ are essentially very great and strikingly observable when viewed through diaphragms, and so *uncontrasted* with the splendour of the general solar surface. The intense brilliancy of the calcium light becomes relatively a black spot when projected on the Sun's disk.

† More poetically—*ποντίων τε κυμάτων ἀνήριθμον γέλασμα.* (Æsch. Prom.) A similar scene is described by Mr. Huggins as "not unlike the stormy sea."

‡ *Monthly Notices* for June, 1869.

approximated in shape to a reaping-hook bent towards the west. The basic parts were seen to be intruded upon by yesterday's penumbra, and the penumbra in its turn was invaded by an advance (eastward) of the photosphere, which was abutting against the middle of the western side of the ellipse. The whole of B was curved in the same westerly direction, and the apex of A (on the N.E. side of the ellipse) had assumed also a bent form towards the west. With the diaphragm the terminal of C was observed to be bifurcated, with the westerly branchlet carried also towards the west. Since yesterday B had become narrower throughout its length, drawn out by the curvilinear motion. The western penumbra, forming the base of C, assumed also a curved shape towards the west. Very great disturbance visible on the south and south-easterly sides of the ellipse.

Thus evidence appears to be afforded by these two days' observations of the commencement of vorticose currents in the penumbra and its adjuncts, with a direction from right to left, forming a curve in the upper division of the ellipse, and the set of the photosphere from west to east.

Sept. 22. Definition exceptionally good. The large elliptical penumbra was now perceived to be converted approximately into two circular ones, which for convenience may be termed northern and southern divisions. C is gone, swallowed up by the advanced curved motion of the western penumbral masses. The greater part of B is now become straight, and only towards its southern apex does it still curve towards the west, making part of the southern circular boundary of the northern division, while the bases of the tongues (A and B) appear to have merged to form a portion of the south-east circular coast of the southern division, and curved eastward. Presumptive evidence of another vorticose movement east and south-east of the former.

The straight (and recently attenuated) part of B was more luminous than even the general photosphere, and I considered it to have been floating at a very high elevation, and thus out of the circular movement of the lower current, while the bent part of it (towards the west) partaking more of the penumbral tint (inferentially at a lower level), together with the basis of A and B (attached to the northern penumbra), would thus be under the direction of the lower current. There appeared to be great activity about this locality, and at good moments of observation I could perceive two wisps of the penumbra like windy cirri; one in its divided drawn-out parallel form, seemed to be a portion of the boundary of the northern part of the lower umbra, and the other normally curled and drawn in towards the central parts—indications of lateral and vorticose currents.

On the surface of the eastern penumbra great elevations were visible, with peaks as brilliant as the straight portions of B before mentioned, and the photosphere on the east was full of large billows, while round the margin of the penumbra the photosphere also showed similar forms, both by direct and reflected

vision. A delicate line (D) on the east of the larger part of the straight B (and projected like B from the margin of the northern penumbra) was visible when the solar glare was excluded by diaphragms and the view limited and localized.

Sept. 23. Definition very good; the wind still easterly. The straight tongue B, with its apex turned yesterday to the west, was now found to be turned to the east, and the high photosphere of the west had apparently glided down and set in *en masse* for a *curved* flow towards the east, as the western penumbra at the base of C had previously curved towards the west. The delicate line D is still on the east of B, and not altered either in position or size—a sort of stratus-cloud indicating complete calmness locally, while the lower strata are bent round towards the east; showing that vorticose currents had extended so far towards the east, and indicating also both the existence and direction of the motion at lower levels, where the nebulous or gaseous matter was broken up into *curved wisps of smaller diameters.*

Now on the photosphere, all around what was the original and unbroken penumbra (and embracing both its subordinate and included ones), elevated ranges were to be seen whose topmost ridges and apices reflected the same degree of splendour as the northern part of B, and like the penumbral eminences of the previous day. I also noticed that one of these ranges on the easterly coast was bifurcated, and while one branch followed the trunk-range which formed the upper eastern border of what was the original great ellipse, the other branch turned off and took a *curvature* inwards, and formed part of the curved portion which dimidiated the ellipse.

These two days' observations will have led us to infer that a calm pervaded some of the higher regions, while energetic movements were going on in the lower, and that gyratory motions in a *contrary* direction had commenced in the eastern locality, and (on scrutinizing the directions and size of the circular forms) it also appeared that such curvatures had contracted their diameters, and that those on the north-west quarter had taken a direction from right to left, while in the south-eastern quarter the direction was from left to right.

Sept. 24. Found B still curved towards the east, but now more throughout its length, while the delicate line D was still unaltered. In this locality I could also make out, on careful survey, a minute fleecy line attached to D. This more diminutive object only became visible when the Sun's direct surface-brightness was shut out by diaphragms, conjointly with the exclusion of all indirect and refracted daylight by enwrapping (photographer-like) both head and telescopic eye-piece in an impervious India-rubber cloth. This (when thus viewed) exhibited interrupted brilliant patches, like the cusps of the early Moon, with their outlying enlightened highlands and intermediate unenlightened valleys, and, therefore, would lead to the inference that this solar branchlet occupied different levels at different points; in fact, was

of an undulating form.* I should add also, that this connected projection was clearly attached to the northern penumbra, and towards its southern portions it displayed photospheric brightness. It must thus have been an *undulating incline* rising up from the lower penumbral to the higher photospheric level. It might also be remarked that, while horizontal currents alone, when acting upon a gross material, would indeed produce an undulatory movement without the aid of ascensional currents (because in such a case the varying outline would be the *resultant* of horizontal and gravitating forces), yet as the rarity of the gaseous envelope precludes the idea of density sufficient for such effects, and we have already clear evidence of the tilting up towards the south of this luminous line, we may also with less scruple infer that this upward and undulatory movement might be the result of intermittent or spasmodic action from beneath.

Great changes, however, had been going on elsewhere. The body of the photosphere had rapidly set in from the elevated regions on the west and north-west and overspread the central portions of the divided ellipse, and had carried back A towards the east to straightness.

Now just south of this locality were portions of the photosphere to be seen overhanging the penumbral cliffs in a jagged outline, and projecting horizontally to a greater or less extent over the subjacent penumbral talus. The further development of a somewhat similar scene was witnessed by Pastorff in 1828. These features seem to suggest an existing *lateral* motion in high solar regions. The general and now complex currents had resulted in the breaking up of the upper half of the original ellipse into three compartments and of the lower into four.

These grand movements of the photosphere were however, I conceive, no more the actual *initial* cause of the vast changes thus superinduced over the once elliptical area than is the slow and irresistible march of the giant glacier the initial cause of its destructive consequences. Both are only proximate causes, and are themselves the results of a deeper and more hidden power. In the present instance a range of circular orifices on the western regions, and eruptive symptoms of a similar nature on the eastern, left no doubt in the mind of the observer as to the immediate source of the great photospheric disturbance now in progress.

Sept. 25. Outbursts more developed around the entire district, and while the general shape of the original ellipse (now divided and subdivided) is still marked, yet the connexion of the *sequence* of these changes could not now be traced, and therefore ceased to be of real instructive value.

Sept. 26. Disturbing causes became still more visible and general. The northern division now contains only two openings

* Large Newtonian Reflectors have decidedly great advantages over large Refractors for direct observations of the Sun, as the eye-piece, being placed at right-angles to the tube, allows the observer so to manage his dome-shutters as always himself to be in the shade.

and the lower three. The lower division is also completely severed, not only by the intrusive photosphere, but by an almost circumscribing chain of larger orifices, which eventually increased, and produced a virtual detachment of the two divisions. I noticed a long and thin tongue in the northern penumbra, but I could not venture to identify it with any previously existing one; and the definition to-day not being so good, I was also unable to speak decisively either as to its absolute connexion with the opposite penumbra, or as to any variation of brightness throughout its length.

In the present state of heliographical knowledge I would rather note what appeared to be observed facts respecting photospheric currents than assume any theory about their hidden and originating causes, whether intrinsic, from the expansion of gaseous matter; or extrinsic, from disturbing planetary attractions; or from the conjoint action of both such influences. In the chronological account of the last described solar district and its successive changes I have not met with any indications of a *downward* current over the umbra. But as the evidence in this particular case is simply of a negative nature, no general inference can be drawn against the possible occurrence of such at other times and over other spots. The gyratory movements described above were, I think, truly cyclonic in the strict meteorological acceptance of the term.

Every gyratory movement in our atmosphere is not indeed cyclonic, for such may arise from the action of lateral aerial forces proceeding from opposite directions, and producing whirlwinds combined with ascensional currents. Where, however, we have reason to infer that ascending currents are the *cause*, and not the *result* of such motion, then we may safely term such currents cyclonic.*

Now we have given (apparent) proofs in the earlier part of this Paper of the existence of *ascensional* currents over, and around, and within solar spots. We have also seen that the disturbance of the elliptical spot was in existence *anterior* to the development of the vorticose movement, so that with such an upward tendency of the Sun's envelope, combined with his own axial revolution, we have the two essential elements for cyclonic currents on an extended scale.† We have also noticed the apparent decrease of the diameters of these vorticose movements in the lower regions, which would seem to indicate an inverted conical or funnel-shaped disturbance, and likewise a revolving of such in opposite directions. These solar phenomena impress the mind

* "Whirlwinds are whirlwinds of compression, whereas cyclones are whirlwinds of rarefaction." See Sir John Herschel's masterly article on Meteorology in the *Encyc. Brit.*

† The velocity of rotation at the Sun's equator is 4415 miles per hour, while that of the Earth is only 1019 miles. (See Revised Tables in Donkin's "Astronomy.") Terrestrial cyclones are thus upon a comparatively small scale. Ordinarily from 2 to 300 miles diameter, and only sometimes exceeding 500 miles.

with the idea of an existing analogy between terrestrial and solar meteorological laws, and the idea (as suggested by Sir John Herschel) is strengthened by the well-known arrangement of the great display of macular zones on either side of the Sun's equator and by the freedom (comparative or absolute) from such photospheric outbursts on other parallels—corresponding so much with the presence or absence of cyclonic and extensive aerial disturbances in our own atmosphere. On the other hand, we find that the *reversed* motions of terrestrial cyclones occur respectively in the northern and southern hemispheres (separated as they are by the equatorial belt of calms),* and have a normal direction, north of the Equator from right to left, and south of the Equator from left to right, while in the above-described solar cyclones both appeared to move in reverse order, although in the same (northern) hemisphere.

Now under all these physical circumstances I am led to think that the due consideration of such analogies, and of any apparent exceptions, may become a means of drawing important inferences respecting the position of the source of solar disturbances. If cyclonic movements be carefully watched and described, and it should result, after a long period of observations, that there is a normal direction for such movements (*i.e* reversed in the different solar hemispheres), and that the vorticose movements around two centres (observed in the elliptical spot) have been an exceptional case or mistakenly described,—then we should rather infer that the grand comprehensive *influence* was from *without*, and operating upon the general surface of the photosphere. But if, on the contrary, the cyclonic movement should prove to be abnormal as compared with the terrestrial, and it should appear that I have not misinterpreted the changes observed, then we should be led rather to infer that such influence was from *within*, operating locally and not generally.

Yet contemporaneously with such observations we should also, I think, always direct our attention to the unbroken expanse of the photospheric ocean, and scrutinize those extensive and high heapings-up whose surface may never be scarred by any intrusive macular openings, and where, indeed, we should most probably be watching the very results of *external influences*. Above all, we should remember that not by hastily drawing inferences from one source exclusively, but by judiciously weighing and harmonising the evidences suggested by all the phenomena, can we even reasonably hope to attain to any valuable insight into the causes of the disturbance of the solar envelope, and be led to discern whether such be really simple or be not rather complex.

Note on Solar Faculæ.—The deep valleys beneath my Observatory are occasionally filled with a dense stratus-mist whose *upper* portions are subsequently converted by a cloudless Sun into every variety of the nebular forms except the "*nimbus*."

* Vide "Physical Geography of the Sea," by Com. Maury, U.S.N. passim.

In the central parts of the valleys where the mist attains a depth exceeding 500 feet the first movement visible is a ridging or furrowing of the stratus producing the "*cumulo-stratus*." When these furrows are most numerous and inosculating, and are viewed reflecting from their *upper* surface their dazzling sun-lit brilliancy and light-shadows, I recognise at once the most exact terrestrial counterpart of the solar photospheric *faculæ*.

Ensleigh Observatory, Lansdowne, near Bath,
27 Sept. 1870.

Note on the use of Eye-screens in Telescopic and other Researches. By Richard A. Proctor, B.A.

Most observers with the telescope keep the unemployed eye open, while studying with the other the features of an object in the telescopic field of view. Closing the unemployed eye produces, in fact, palpably injurious effects. But I suppose every one must have noticed how much better the features of a faint object are seen, when with the hand, or any screen, the unemployed eye is protected,—even though it be only from the luminosity of the star-lit sky, I find that much better results are obtained when the unemployed eye is guarded from the access of all light whatever by a screen lined with black velvet. Yet better vision is secured if the screen be so contrived as to guard the employed eye, also, from all light except what reaches it from the object. This can easily be managed by having a screen covering both eyes, but with sliding doors, by which a small circular aperture opposite either eye can be opened or shut at will. Each aperture can have outside it a small tube passing over the eye-tube of the telescope. Or (preferably for some purposes), a small sleeve-curtain with an elastic circular opening at one end can be attached, at the other, over either eye-aperture. Then if the opening be passed over the eye-tube and suffered to close by its elasticity, the desired result will be secured.

From my own experience I feel confident that for certain special orders of research the use of eye-screens of this sort (the details being arranged according to convenience) will be found of great service. I may particularise—observation on moonlit nights, the examination of faint nebulæ or the search for them, the search for minute points of light, delicate spectroscopic and polariscopic researches, and the investigation of questions concerning the colours of the planets, of the lunar regions, &c. The following description of the *modus operandi* will serve to supplement the above general sketch. The equatorial being set on any delicate object, the observer fastens the screen over his eyes, attaches the sleeve curtain over the eye he proposes to work with, slides the elastic ring over the eye-tube, and suffers it to close round that tube. Having seen that the object is in the

field, and the rate of driving just, he closes both the eye-doors, keeping his eyes open and directed on the perfect blackness of the velvet lining. When he thinks the eye sufficiently prepared, he draws out the sliding eye-door over the telescope-eye, and is then able to apply the full powers of his eye-sight (under exceptionally favourable conditions) to the examination of the object he has to deal with.

I am desirous of learning how far the efficiency of such contrivances depends on aperture,—that is, whether the performance of large telescopes is as much improved as that of smaller instruments. If any Fellows of the Society should care to experiment on this matter (which is not unimportant, I conceive) they would be conferring a favour on me by communicating the results they may obtain. I refer here simply to the comparison between the work of a telescope used in the ordinary way and when supplemented by eye-screens.

I may renew here my suggestion of the great importance of applying such contrivances during the examination of the corona in total solar eclipses, whether with the telescope, spectroscope, or polariscope. As regards the spectroscopic analysis of the corona, any method by which the visibility of a very faint continuous spectrum can be increased (and I imagine that eye-screens such as I have proposed would have such an effect) cannot but be well worth trying. It seems highly probable that the variations in the accounts given so far depend wholly on the amount of opening in the slit,—a narrow slit giving a very faint continuous spectrum and three somewhat brighter lines (one can scarcely speak of bright lines in this case) which are under these circumstances very difficult and delicate objects of observation; a somewhat wider opening gives a continuous spectrum, bright enough to obliterate two of the lines, but leaving one (the one Professor Harkness saw) fairly visible; a wider opening shows only a continuous spectrum, the lines being all obliterated. This at least is the explanation which seems alone consistent with the principles of spectroscopic analysis, and it is obvious that if it be just, any increase in the eye's power of appreciating faint light must be a great gain.

On a Point of Egyptian Chronology. By C. Abbe, Esq.

My limited acquaintance with the recent researches into ancient chronology, and with the standard works on this subject, necessitate my stating that the object of the present Note is to direct more exact attention to a subject that has been touched upon in a general way by others.

In the *Monthly Notices*, vol. xxiv., Mr. Williams gives a list of ninety-two eclipses observed in China from 720 B.C. to the year 0. These we may assume to have been observed in various parts of

the empire, with the unaided eye, and without having been approximately predicted; and may be taken as a fair exhibit of the result of similar observations made by the astronomers of other ancient nations.

Bunsen, in his *Egypt*, vol. i. p. 14, quotes from Diogenes Laertius (who flourished about the year A.D. 200), that the Egyptian records embraced 373 solar and 832 lunar eclipses. By comparing this with the preceding statement of Mr. Williams, we may arrive at an estimate of the number of years that must have elapsed since the beginning of the Egyptian record, assuming only that the climate and other conditions equally favoured both the Chinese and the Egyptian observers.

A simple proportion gives for the 373 Egyptian eclipses a period equal to 4·05 times the 720 years of the Chinese record, or about 2920 years.

It is not probable that the records of the Egyptian astronomers were seriously interrupted by the wars and conquests, and as the Ptolemies fostered our science, I should esteem it probable that the lists quoted by Laertius were complete up to his time, this gives us B.C. 2720 for the beginning of the record. This date would be put back still earlier if the Egyptian records were imperfect.

Now Bunsen puts the seven years' reign of Asses or Aseth at 2782 to 2775 B.C., in whose time the Egyptian year was instituted. It may then be surmised that from his time forward a record of eclipses was kept as unbroken as possible, although it would have appeared more plausible that such record should have begun 276 years earlier, *i.e.* in the reign of Meves. A detailed study of the eclipses visible in Egypt, and the exclusion of those that were too small to have attracted notice, would settle this point.

I have made the following attempt to determine the *minimum* interval required for the accumulation of the Egyptian observations. The *Nautical Almanacs* from 1834 to 1870 (the only ones accessible to me) show that there were during that interval 86 solar and 57 lunar eclipses, of which respectively 12 and 30 were visible at Alexandria. Comparing these with the statement of Laertius, we deduce two independent determinations of the interval, *i.e.* respectively 1120 and 998 years. It also appears that there were about 240 solar eclipses in the 720 years, within which the Chinese recorded only 92.

It would be interesting if a comparison could be instituted between the lunar eclipses enumerated by Laertius and those of the Chinese.

Cincinnati,
August, 1870.

Observations of the Occultation of Saturn by the Moon, Sept. 30, 1870. By C. L. Prince.

I observed the occultation of *Saturn* by the Moon this evening under favourable circumstances, with the exception of the presence of an easterly wind, which always interferes, more or less, with sharp definition. As the Moon approached the planet a very striking difference was observable in their relative brightness; and this difference was still more marked at the time of reappearance when the planet assumed a very pale green colour.

At the moment of contact, as well as throughout the occultation, there was not the slightest distortion of either body; but I noticed that the edge of the ring lingered somewhat upon the Moon's limb about the time of disappearance. The ring, however, maintained a well-defined edge to the last.

The following times of immersion and emersion are tolerably correct, considering the prevalence of an easterly wind, and the very low altitude of the planet,—

Immersion.	h	m	s	
First contact with ring	18	41	28	L.S.T.
Disappearance of ring	18	41	45	
First contact with ball	18	41	53	
Ball occulted	18	42	30	
First contact with inner edge of ring	18	42	37	
Disappearance of ring	18	42	55	

Emersion.	h	m	s	
Reappearance of ring	19	55	18	L.S.T.
Reappearance of ball	19	55	42	
Reappearance of following limb	19	56	14	
Reappearance of inner edge of ring	19	56	22	
Latest contact	19	56	39	

I employed my Tulley Equatoreal of 6·8 inches aperture, and 12-feet focal length. Power 250.

Observatory, Uckfield,
Sept. 30*th* 1870.

Observations of Coggia's Comet. By S. J. Perry.

The comet discovered this year by M. Coggia of Marseilles has been observed here during the month of October, and the following positions determined,—

Oct.	G.M.T.	Star of Comparison.	R.A. h m s	N.P.D. ° ′ ″	Comet $d\alpha$ m s	$d\Delta$ ° ′ ″
2	12 50 A.M.	α *Andr.*	0 1 42·98	61 37 21·9	+ 9 6	+1 8 0
2	10 9 P.M.	,,	,,	61 37 21·7	+ 3 16	+0 51 20
3	9 5 ,,	,,	,,	61 37 21·5	− 4 10·5	+0 20 10
4	7 25 ,,	,,	,,	61 37 21·3	−11 58	+0 0 45
9	7 50 ,,	Star *a*	23 14 30·3	60 17 42	+ 1 50	+0 8 0
9	11 35 ,,	,,	,,	,,	+ 1 18	+0 6 40
10	10 39 ,,	Star *b*	23 9 56	60 0 4	− 0 32	+0 15 0
14	8 42 ,,	Star *c*	22 50 30	59 50 36	− 1 15	+0 0 15

The Comet was also observed on the 20th, but as only an approximate position could be obtained on account of the badness of the weather, I have not entered the result. Throughout the latter half of the month the clouds have rendered observations almost impossible.

Stars *a*, *b*, *c*, are taken from Argelander's large maps, and have been reduced to Oct. 1870. Refraction does not sensibly alter $d\alpha$ and $d\Delta$.

The concluded positions of the Comet are

	G.M.T. h m	R.A. h m s	N.P.D. ° ′ ″
Oct. 2	12 50 A.M.	0 10 49	62 45 22
2	10 9 P.M.	0 4 59	62 28 42
3	9 5	23 57 32	61 57 32
4	7 25	23 49 45	61 38 6
9	7 50	23 16 20	60 25 42
9	11 35	23 15 48	60 24 22
10	10 39	23 9 24	60 15 4
14	8 42	22 49 15	59 50 51

Stonyhurst Observatory.

The Laws according to which the Stars Visible to the Naked Eye are distributed over the Heavens. By Richard A. Proctor, B.A. (Abstract.)

The method briefly described in the Supplementary No. of the *Monthly Notices*, vol. xxx., has led me to the following general results.

I find that the Milky Way, both in the northern and southern hemisphere, is singularly rich in stars, and in each hemisphere severally it is the richest special region of all. In the northern* there is a very rich region covering *Cygnus*, *Cepheus*, *Ursa Minor*, and *Lacerta*. It is numbered 2 in the accompanying Table. In the southern hemisphere there is a corresponding rich region

* Several copies of a map illustrating the distribution of all the lucid stars over the heavens were exhibited at the Meeting. The map belongs to the second edition of my "Other Worlds."

covering the keel of *Argo*. It is numbered 12 in the Table. But nearly half the northern heavens (the more central portion) is exceptionally rich in stars, while even in a more marked degree one half of the southern heavens (almost centrally surrounding the Greater Magellanic Cloud) exhibits exceptional richness. These are numbered 3 and 11 in the table. The remaining portions of the two hemispheres form the parts numbered 4 and 10 in the table. Certain parts of this outer and poorer region are exceptionally poor in lucid stars. They abut on the richest parts of this rich region, and the contrast is thus rendered more striking. They are numbered 5, 6, 8, and 9 in the accompanying table. It will be noticed that none of the areas included in the table are very small, except only those forming gaps and lacunæ in the Milky Way. For convenience many of the regions have been limited to a certain size, viz., $\frac{3}{22}$nds of the hemisphere.

	Name of Region.	Area. Area of Hemisphere as 1.	Number of Lucid Stars.	Richness. Average=5850.
Northern.	1 Milky Way ..	$\frac{1}{11}$	497	9940
	2 Richest region ..	$\frac{3}{22}$	622	9050
	3 Central region ..	$\frac{5}{11}$	1420	6248
	4 Outer region ..	$\frac{6}{11}$	1070	3923
	5 Poor region I. ..	$\frac{3}{22}$	201	2948
	6 Poor region II. ..	$\frac{3}{22}$	175	2567
	7 Gaps in Milky Way ..	$\frac{1}{62}$	20	1240
Southern.	8 Poor region I. ..	$\frac{3}{22}$	161	2361
	9 Poor region II. ..	$\frac{3}{22}$	216	3198
	10 Outer region ..	$\frac{1}{2}$	893	3572
	11 Central region ..	$\frac{1}{2}$	2467	9868
	12 Richest region ..	$\frac{3}{22}$	895	13126
	13 Milky Way ..	$\frac{1}{10}$	618	13596

The numbers in the last column indicate the total number of stars which would be visible to the naked eye, if the whole heavens were covered as richly or as sparsely as the corresponding region.

This Table proves that special laws of aggregation and segregation exist among the lucid stars. The fact that the Milky Way is also shown to be exceptionally rich in lucid stars, while its gaps and vacuities are exceptionally bare, seems to dispose beyond all question of the theory that the stars forming the milky light of the Galaxy (regarded as a whole) lie at vast distances beyond the lucid stars.

It may be mathematically demonstrated that even the less marked feature, the exceptional richness of the southern hemisphere, cannot be ascribed to chance distribution; thus—

The northern heavens contain 2490 lucid stars, the southern

3360, or 5850 in all (we need not inquire whether these numbers are exactly correct, because the laws of probability include chance errors as well as chance distribution). The probability that a star placed at random will lie on one or other hemisphere equals $\frac{1}{2}$. Hence the antecedent probability that out of 5850 stars so distributed as many as 3360 will be found in either hemisphere is represented by a fraction whose numerator is the sum of all the coefficients in the expansion of $(x+y)^{5850}$, in which either x or y is raised to a power of not less than 3360; and its denominator the sum of all the coefficients, or $(2)^{5850}$. The sum of the favourable coefficients is

$$= 2\left[1 + \frac{5850}{1} + \frac{5850 \cdot 5849}{1 \cdot 2} + \&c. \ldots + \frac{5850 \cdot 5849 \ldots 3361}{1 \cdot 2 \ldots 2490}\right];$$

and the sum of the unfavourable coefficients is

$$= \frac{5850 \cdot 5849 \ldots 2926}{1 \cdot 2 \ldots 2925} + 2\left[\frac{5850 \cdot 5849 \ldots 3360}{1 \cdot 2 \ldots 2491} + \frac{5850 \cdot 5849 \ldots 3359}{1 \cdot 2 \ldots 2492} + \&c. \ldots + \frac{5850 \cdot 5849 \ldots 2927}{1 \cdot 2 \ldots 2924}\right].$$

The total number of favourable terms is 4982, that of the unfavourable 869, or more than one-sixth of the former. Hence by comparing the largest unfavourable term with six times the largest favourable term, we shall get a ratio obviously less than that we require. (It needs only a consideration of the law according to which the coefficients increase to see this.) Hence the odds against the occurrence of the observed arrangement of the stars, as respects the northern and southern hemispheres, would be, if chance-distribution were alone in question,

$$> \frac{1}{6}\left\{\frac{\frac{5850 \cdot 5849 \ldots 2926}{1 \cdot 2 \ldots 2925}}{\frac{5850 \cdot 5849 \ldots 3361}{1 \cdot 2 \ldots 2490}}\right\}$$

$$> \frac{1}{6}\left\{\frac{3360 \cdot 3359 \ldots 2926}{2491 \cdot 2492 \ldots 2925}\right\}$$

and obviously therefore (*à fortiori*)

$$> \frac{1}{6}\left\{\frac{2926 \cdot 3013 \cdot 3100 \cdot 3187 \cdot 3274}{2925 \cdot 2838 \cdot 2751 \cdot 2664 \cdot 2577}\right\}^{87}$$

Now the value of this expression is easily calculated to be

66,996,090,000,000,000,000,000.

Hence the antecedent probability of the observed event is less than

$$\frac{1}{66{,}996{,}090{,}000{,}000{,}000{,}000{,}000}.$$

The denominator would have had 132 figures had we dealt with the probability of the existence of two such regions as those numbered 3 and 11 in the above table.

As respects this last relation it may readily be shown that if *a sphere having a radius exceeding many million times the distance of the furthest object revealed by the Rosse Telescope, were filled with atoms severally minuter many million-fold than the minutest object which the microscope will reveal, the chance that a specified atom would be selected at random from that inconceivably vast universe of atoms, would be many million times larger than the antecedent probability of the observed relation, supposing chance-distribution alone in question. Yet that relation is by no means the most remarkable exhibited in the above Table.*

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